UNITED STATES DEPARTMENT OF COMMERCE
National Oceanic and Atmospheric Administration
NATIONAL MARINE FISHERIES SERVICE
National Marine Fisheries Service
Northwest Region
7600 Sand Point Way N.E., Bldg. 1
Seattle, WA 98115

Northwest Fisheries Science Center
2725 Montlake Boulevard East
Seattle, WA 98112-2097

February 14, 2000

Mr. Jeff Curtis
Trout Unlimited
West Coast Conservation Office
297 G Avenue
Lake Oswego, OR 97034

Mr. Rob Masonis
American Rivers
NW Regional Office
150 Nickerson St.
Suite 311
Seattle, WA 98109

Mr. Todd True
Earthjustice Legal Defense Fund
203 Hoge Building
705 Second Avenue
Seattle, WA 98104

Dear Sirs:

Thank you for your letter of January 25, 2000, regarding the Northwest Fisheries Science Center's Cumulative Risk Initiative (CRI). We believe an open dialogue between CRI scientists and all individuals, groups and agencies commenting on their work is valuable to, and in fact an important part of, improving the CRI's approach to assessing risk in listed salmonid populations.

In the enclosed response, CRI scientists reply in detail to each of the seven questions raised by Dr. Oosterhout. They also summarize important points emerging from her comments on the CRI's extinction risk, and document specific changes they have made to improve the scientific rigor of these analyses. An even more detailed response will come in the form of the revised and improved CRI analysis that Dr. Kareiva mentioned in his letter of January 24, 2000.

In responding to Dr. Oosterhout's comments, CRI scientists were quite pleased by the very fact that Dr. Oosterhout could duplicate CRI analyses in order to conduct her critique. This illustrates exactly the situation for which National Marine Fisheries Service (NMFS) is striving. NMFS seeks an approach that is sufficiently transparent where independent scientists can run different data sets through the analyses or test alternative hypotheses and scenarios. Apparently, CRI scientists are succeeding at achieving such transparency.

We appreciate your continuing interest in our work. If you have any questions regarding our response to Dr. Oosterhout's report, please contact Dr. Michelle McClure at the Northwest Fisheries Science Center.

Sincerely,

William Stelle	Usha Varanasi
Regional Administrator	Science and Research Director
National Marine Fisheries Service	Northwest Fisheries Science Center

Enclosure

cc: Dr. Peter Kareiva
George Frampton, CEQ
Anne Badgley, USFWS
Brigadier General Carl Strock , U.S. Army Corps of Engineers
Don Sampson, CRITFC
Judi Johansen, BPA
Northwest Power Planning Council Members
Chip McConnaha, Northwest Power Planning Council
Dr. David Marmorek
Jim Lichatowich, Chair of the ISAB
Daniel Goodman, ISAB
Bill Shake, USFWS
Danny Consenstein, NMFS
Rick Ilgenfritz, NMFS
Roy Hemmingway, Oregon Governor's Office
Curt Smitch, Washington Governor's office
Ed Bowles, Idaho Department of Fish and Game

Response to:
Seven questions about the Cumulative Risk Initiative (Gretchen Oosterhout, Ph.D.)

Prepared by:
Peter Kareiva, Ph.D.
Christopher Jordan, Ph.D.
Michelle McClure, Ph.D.

1) What justification is there for defining quasi-extinction as one fish in a single year?

The important issue is not the numerical value at which quasi-extinction is assessed so much as the consistency of its application across all populations within the analysis. Certainly using a higher threshold will increase the apparent extinction risk, but the point of our analysis is not to predict the precise year in which a population goes extinct. Rather, we seek to compare the relative risk across large geographic areas (the entire Columbia River Basin in this case) and multiple distinct population segments (12 ESUs). However, in response to feedback from our technical workshops and to increase the potential sensitivity of our analysis, the CRI no longer uses a single quasi-extinction threshold of one fish. Instead, the CRI now uses thresholds of one fish (true extinction, in our modified analyses), fifty fish (a value compatible with previous quasi-extinction thresholds both in and out of the Columbia Basin (Nicholson and Lawson, 1998; Upper Columbia QAR process)), and a 90% decline from current abundance (a measure of risk independent of absolute population size).
These values may appear at first glance to be more conservative than some of those cited by Dr. Oosterhout. However, many of the thresholds cited in Dr. Oosterhout's report pertain to species or independent populations, not to open stocks that can be revitalized by migration. None of the populations that the CRI is considering are closed; many have gone to zero historically and then rebounded. Thus, we would be violating the assumptions underlying these analyses were we to adopt the thresholds appropriate for demographically closed systems. Additionally, quasi-extinction thresholds arise from the detrimental demographic effects of small population size. Unfortunately there is no good data on the precise level at which depensation occurs in naturally breeding salmonid populations. Therefore, we feel it most justifiable to use multiple thresholds, including one that is not strictly abundance based. That is, rather than focusing on what may be viewed as conservative predictions of risk, we stress the conservative nature of our method as the one most readily supported by the data available.

2) What justification is there for using an average growth rate to conduct risk assessment?

CRI extinction analyses did not, in fact, use an average growth rate. Rather, extinction risk analyses use the entire range of growth rates for the time period analyzed, and fit a line to those growth rates. However, Dr Oosterhout does raise an important point. One of the assumptions underlying a Dennis extinction risk analysis is the assumption that trends are linear, or homogeneous in time. Although the populations themselves may be increasing or decreasing, there should be no trend in the rates of decline or increase, such that the rate of decline is getting progressively worse or better. Both the Independent Scientific Advisory Board and Dr. Oosterhout (as well as CRI scientists) have noted that stocks of spring/summer chinook salmon in the Snake River are decreasing at an increasing rate (i.e., the trend is not linear). Unfortunately, dealing with such trends appropriately is a complex statistical issue. Until more appropriate extinction risk analyses can be developed, CRI scientists are incorporating this trend into their analyses by assessing extinction risk using the variance derived from the entire time series and the population growth rate derived from the last five (worst) years of the time series. This is the most conservative estimate of extinction risk that can be made for the Snake River spring/summer chinook ESU as it maximizes the variance and uses a minimum estimate of the population growth rate. Leslie matrices are used only in sensitivity analyses. In other words, they are used to determine the expected change in population growth rate with a change in a demographic parameter such as fecundity or survival at a particular life stage. Therefore, the lambda values in these analyses do not contribute to an assessment of risk, but rather, to a determination of expected responses to changes in demographic parameters. It is true that the lambda calculated from these matrices is the true population growth rate only if the population is at its stable age distribution. However, even in situations where the population is not at the stable age distribution, the actual population growth rate is correlated with lambda, making these types of sensitivity analyses valid. Finally, although the CRI has, to date, plotted only mean sensitivity results, our analyses have considered variation in underlying parameters, including lambda. In the future, we will include both mean and standard error of these analyses in our graphs and tables in order to prevent any confusion. Results from sensitivity analyses conducted thus far are robust to variation in these data.

3) Why use the 1980-1990 brood year data for the analyses when previous analyses used 1980-1993?

We agree with Dr. Oosterhout that using the most recent data available is of critical importance in assessing the risk faced by imperiled species. Data for some (but not all) index stocks had been provided to the CRI for 1991-1993 brood years. These data were initially used, and the analyses posted on the web. However, these analyses were withdrawn only because the data upon which they relied for those later brood years were not adequately documented or attributable to a source. Updated CRI analyses relies on data from Beamesderfer *et al.* (1997), which is complete through brood year 1990 only. We have received the most recent data available (through brood year 1994) from the Oregon Department of Fish and Wildlife only within the last month. These new data included revisions to past spawner estimates in several index reaches as well as overall updates. We are working vigorously to re-do all analyses using the entire data set available.
NMFS is committed to using the best, most well-documented and attributed data it can obtain, as well as to updating our analyses as soon as new data are available, so that the results will be available to assist decision makers in a timely manner.

4) Why does the CRI rely on a questionable sensitivity analysis method?

Dr. Oosterhout objects to the use of a standard reduction in mortality across all life stages as a sensitivity analysis, preferring instead the more “textbook” elasticity approach. However, the references Dr. Oosterhout cites may not deliver the message she implies, and the method used by CRI is widely accepted. For instance, much of the original mathematical formulation underlying elasticity analyses is a foundation for life history theory, to assess the strength of selection on different life stages. Therefore, elasticity analyses will always indicate that there is a greater response in population growth rate to life stages with high reproductive value since these are the life stages at which selection is strongest. However, in conservation applications, the strength of selection at particular life stages is not always the most appropriate question. For example, since many management actions aim to reduce mortality, determining life stages at which these reduction are likely to have the largest impact on population growth rate is an important first step in assessing the likely efficacy of those actions. It is widely accepted that numerical experiments of the type conducted by CRI scientists are a necessary ingredient of any conservation analysis. Importantly, we are in contact with the leading authorities in the field and are very aware of the subtleties of using matrix models. In fact, Dr. Kareiva has received from Professor Caswell preliminary notes from his new book regarding matrix models in conservation. Dr. Kareiva himself is an authority in the area, having published several papers that apply matrix models to conservation issues.

5) Why does the CRI underestimate post-Bonneville mortality, and over-estimate first year mortality instead of using values from available literature and PATH?

This critique is somewhat perplexing. Dr. Oosterhout asserts that smolt-to-adult return values (*i.e.*, post-Bonneville survival values) used by CRI are much higher than the available literature and PATH support and that this over-estimation, in turn, causes the first-year survival to be underestimated. Finally, she asserts that these estimated parameters reduce the importance of estuarine survival in the overall conclusions drawn by NMFS and the Army Corps of Engineers, and in particular, that the apparent over-estimation of estuarine survival causes the importance of indirect mortality due to the hydrosystem to be reduced.

Survival rates used in Leslie matrices by CRI scientists were derived from papers published in peer-reviewed journals, PATH documents, and are documented both on the CRI web page and in the Anadromous Fish Appendix. First-year survival (s_1) was determined by solving the Euler equation with run-reconstruction series as input, a well-accepted procedure in population biology. It is true that an alternative approach would be to determine first-year survival from the literature and solve the Euler equation for s_e (estuarine and early ocean survival). In fact, CRI scientists have taken this alternative approach and determined that it has very little impact on the overall results.

Clearly there is enormous mortality during both the first-year (freshwater rearing) and estuarine and early-ocean phases. Comparing the effects of a ten percent reduction in mortality on population growth rate across all life stages indicates that reducing mortality during these two early stages leads to a ten-fold larger increase in growth rate than that obtained by reducing mortality at other life stages. The difference in benefit between the two early life stages is secondary to the enormous difference between their effect on population growth rate and that of all other life stages. CRI has not claimed that one of these early life stages was more important than the other, rather we were indicating, by simulation, the potential value of targeting stage specific management actions. Finally, the distribution of mortality between these two early life stages does not, as Dr. Oosterhout claims, affect the importance of indirect mortality from the hydrosystem. Rather, it is the magnitude of that indirect mortality that is most important.

6) Why has there been no discussion of model validation?

Model validation of extinction risk estimates implies sending some populations to extinction as a test of our analyses. This is obviously not an option that we wish to pursue. We feel it much more appropriate to test the validity of our methods by assessing the consequences of our

underlying assumptions, learning from this exercise how to improve our method and refine the match between our methods and key features of salmon biology. It is extremely important to remember that the CRI analysis is not based on a single model but on a chain of analytical steps that we have developed to represent the multifaceted problem presented by estimating extinction risk in salmonid populations. As a result, we assess our confidence in our results by exploring the strength of our assumptions and the sensitivity of our analyses to these assumptions. In a sense, assumption testing assesses our understanding of biological processes and the appropriateness of the analytical tools chosen to represent them in quantitative models. We carry out the process of assumption testing by systematically relaxing assumptions and examining the effect on model predictions. In other words, we determine the biological accuracy of specific model components and make incremental improvements to our over-all method. The CRI approach is to start simple and refine the tools as our understanding of both the process and the model's behavior grows.
To explore and improve the robustness of our approach the CRI has been testing and relaxing assumptions inherent to the biology of Columbia Basin salmonids and the Dennis extinction risk model. For example, we have explored the consequences of several assumptions inherent in previous life cycle and extinction modeling of salmonids: density dependence, environmental variability and demographic stationarity. The CRI has used a bootstrap method to test for density-dependence in all time series and found no evidence of density-dependence in Snake River chinook salmon populations (using data from 1980 onward). This result validates our use of a simple linear matrix approach, simultaneously calling into question the numerous models that have assumed a Ricker form of density-dependence. Similarly, the CRI has re-examined its original "Dennis extinction model" with respect to the estimation of environmental variability, a technical issue that the ISAB review recognized as an important issue. Using a simulation approach, we have demonstrated that the initial "modified-Dennis approach" does a good job of estimating trend, but not variance. As a result, we have developed a new modification of the Dennis approach that gets both the variance and the trend correct. The basic Dennis model assumes that the input demographic time series will be representative of population dynamics into the future. However, if environmental conditions are in flux a long term demographic time series may contain several trends in population processes. For example, salmon populations are strongly influenced by long period ocean condition cycles, resulting in "good" and "bad" phases of population growth. The requirement of constant trends in recruits per spawner data is something the revised CRI analysis also addresses. As mentioned in response to question number two, until more sophisticated methods are developed, (*e.g.*, an analytic method to incorporate non-stationary time series in the Dennis model), the CRI is using the full-1980 onward time series to estimate variance, but only the most recent five years to estimate a worst-case instance of population trend. This is a temporary solution, but a conservative one. It is worth noting that if ocean conditions improve due to a regime shift, recruits per spawner ratios may start to increase and calculations dominated by the "poor phase" of ocean conditions could be labeled too pessimistic.

7) Why is the CRI not using textbook risk assessment methods?

We believe that the CRI is using appropriate risk assessment methods for determining extinction likelihoods. The method employed, first described in Dennis, 1991, has become the standard approach in conservation biology for estimating the probability distribution of time to extinction in discrete populations. The Dennis method uses available demographic time series to estimate a stationary population growth term and the variance in the demographic data to predict the probabilistic bounds on the demographic trajectory. Specifically, the method assumes that the variance in the demographic time series increases as a diffusive process in time. As a result, the population trajectory is not projected into the future, as a matrix population model would do, rather the approach predicts the range of values that the trajectory might achieve through time and the probability of obtaining these values given that current demographic processes still apply. Therefore, there are two key assumptions to this method: that the uncertainty in the population trajectory grows with time and that its growth can be modeled as a diffusive process; and that the demographic processes are stationary such that the present behavior is representative of all points in the future. The CRI, along with the entire conservation biology community, accepts these assumptions and the resultant limitations to the application of the Dennis method, but it does not do so blindly. The CRI is working closely with theoretical population biologists who are explicitly testing the effects of the Dennis method's error model on its prediction of extinction risk. So far there appears to be no reason to question the diffusion model of process error. The assumption of demographic stationarity may appear to be quite dangerous given what is known about the coupling of salmon population dynamics to dynamic environmental conditions, however, the CRI has chosen to use this fact to our advantage. Rather than ignoring the potential for population growth rates to be variable in time, we calculate this term from different segments of the demographic time series to estimate its historic range. Running the extinction risk assessments under a variety of demographic conditions simultaneously tests the consequences of the stationarity assumption, reveals the risk assessment's sensitivity to demographic parameters, and establishes a link between our analyses and on-going work demographic processes to habitat and environmental variation.

Critical to the CRI approach are a strict adherence to the primacy of data and an avoidance of models for which the required input is lacking. It is for these reasons that the CRI has adopted, and subsequently modified for our particular application, the Dennis approach to risk assessment: the necessary and sufficient data to support the model exists for all populations of interest. The CRI philosophy contrasts with Dr. Oosterhout's advocacy of stochastic demographic matrices for risk assessment. The primary data for matrix population models consist of spawner or redd counts through time; these are expanded into run-reconstructions, increasing uncertainty. Additionally, uncertainty arises strictly from fluctuations in spawner counts. Thus one could construct Leslie matrices for salmonid lifecycles and perform stochastic simulations, and in fact one of our scientists has developed a user-friendly model for doing so. However, parameterizing such a model relies more on intuition than on data. For example, one has to guess how variability enters stage-specific rates (limited data), or assume the pattern of covariance among matrix entries (no data). The CRI felt that it was better to match the complexity of methods to the level of information and detail available in the data.

Conclusions

In summary, while Dr. Oosterhout raises several valid points, they are issues currently being addressed by CRI efforts. In general, initial CRI analyses have followed a simple approach for two reasons. First, good science starts simple, and second, simple methods are often all that can be supported by the data. The CRI then explores the limitations and errors of its approach, adjusting methods accordingly (e.g., Dennis model modification, multiple quasi-extinction thresholds). First-cut analyses are not the CRI's final assessment, but rather the initiation of a progression of logical arguments. By making public its early analyses, the CRI has opened this process to non-NMFS scientists' participation. We are confident that the revised CRI analyses will make it clear that we listen to criticism, modifying our approach where necessary.

NWFSC 2/14/00

Additionally, NMFS is compelled to develop methods and tools that can be applied to all twelve ESUs in the Columbia Basin. This is important because region-wide planning will need to weigh trade-offs and priorities for all of these ESUs simultaneously, and we believe that simple comparative metrics and analyses regarding all ESUs will aid in the decision making process. The CRI is committed to transparency and simplicity in a manner that is not always the choice for resource management models. Thus, if there is confusion about our approaches or conclusions it is the responsibility of CRI to do a better job with its communication efforts such that misunderstandings are minimized. For example, the CRI has not updated its web presentations over the last two months while re-doing analyses with new data involving 1991-1994 brood year run-reconstructions. As a result, some of the issues raised by Dr. Oosterhout have already been addressed, in the manner suggested by the report, but were not publicly available for comment. We appreciate the review and feedback Dr. Oosterhout presents.